munication with the perforations in the teeth. Further still, a special instinct leading the snake to make use of this wonderful weapon of offence, and suitable nerves to regulate its complicated action.

Now, unless all these numerous variations—and they might fairly be multiplied by subdivision—had in the first instance appeared simultaneously in one individual, and unless all had been duly connected, the whole apparatus would have been useless, and there would have been nothing of which natural selection could avail itself. Useful intermediate forms there can be none. A rifle is a more formidable weapon than a lance or dart, but of what use would be a thing half-way between the two? The venom-discharging apparatus has in it no part which could possibly be dispensed with.

To give one more instance. The tongue of the woodpecker is moved forwards in a singular way; not simply, as usual, by a muscle and sinew in front of the base of the tongue, but by a sinew terminating in a loop, through which passes another sinew from behind the tongue which, doubling through the loop, is attached to the base of the tongue. By this means, when the muscle is contracted, the tongue is drawn forward with a double velocity, which is to this bird specially useful. Now, it is impossible for any ingenuity to devise an action intermediate between this and the usual simple pull in respect of utility or complexity. But there is much more here than "a particular variation." The first woodpecker that possessed this structure must have had it in complete order, for otherwise the tongue would not move at all. In that woodpecker it must have commenced to exist in a rudimentary form before birth, in a germ possessing novel powers.

And here I must ask, How is it that anyone questions the Duke of Argyll's statement that "all organs do actually pass through rudimentary stages in which actual use is impossible"? Is it not precisely this which is implied in the Darwinian statement that "from the variable constitution of the ovum probably arises the varying structure of the organism developed from it"? What was afterwards developed was at first rudimentary, and useless. This is equally true of the whole organism—say of the serpent, or of the bird—and of the entirely novel and complicated apparatus found in them.

To call the apparatus in either serpent or bird "a particular variation" would be to give up the whole case for Darwinism. A wonderful *combination of many* particular variations has to be accounted for; and, so far as I can see, Darwinism utterly fails to account for it. There are thousands of cases presenting the same difficulty.

There are simpler cases of specific change, in which the concurrence, the simultaneous appearance, of many slight and particular variations is not indispensable, but only their succession in due order in the course of many generations. Here, there is some room for the theory. Thus perhaps, possibly, we might get a giraffe. But I prefer a theory which, if true at all, accounts as readily for the most complicated apparatus as for the simplest forms of living things. R. COURTENAY.

Hotel Faraglioni, Capri, January 31.

PROBABLY many readers of the recent discussion on the transmission of acquired characters will regret that a more definite conclusion has not been arrived at. This is probably due to the fact that the premises now in our possession do not admit of a definite answer yet being given. Those who assume that there is no evidence in favour of the transmission of acquired characters are mostly, I presume, supporters of "the continuity of the germ-plasm" theory of Weismann. Almost everyone admits that individuals may and do acquire certain characters due to change in environment, use, disuse, &c.; but while many maintain that these characters are transmitted to offspring, others deny that such is the case, or think that the evidence is insufficient. In supporting "the continuity of the germ-plasm" theory it is impossible to suppose that the germ-plasm is continued from one generation to another like a portion of entailed property. For each individual gives off thousands of ova or spermatozoa as the case may be, only a very few of which go to produce new individuals; therefore there is a dissipation of "germ-plasm,"—that is to say, in the germinal cells of mammals of to-day there cannot be any of the identical "germ-plasm" which existed in their remote invertebrate ancestors ages ago. For all this dissipation there must be some constructive process, otherwise the germ-plasm would come to an end.

From whence is derived this constructive material? Clearly from the exterior, for a fertilized ovum obtains material from without to admit of growth and elaboration. The constructive material, then, which the "germ-plasm" obtains—to admit of its liberal dissemination each generation—is derived from the external world, *viâ* the organism with which it is incorporated, or indeed of which it forms a part. Seeing, then, that the organism—from which this germinal matter is derived—can acquire characters—that is, undergo certain definite changes in response to altered conditions—then it seems reasonable to suppose that that part of it which ultimately finds its way to the germ-cells, is also modified during its transmission, and will therefore have more or less effect upon the forthcoming generation. But how much variation is due to the above cause, and how much to the almost infinitely various possible combinations of the two unlike germinal elements, it is impossible to say.

J. COWPER.

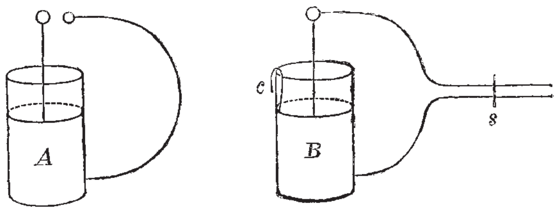
Easy Lecture Experiment in Electric Resonance.

AN experiment, exhibited by me in its early stages at the Royal Institution a year ago, and since shown here in various forms, on the overflow of one Leyden jar by the impulses accumulated from a similar jar discharging in its neighbourhood, is so simple an illustration of electric resonance, and so easily repeated by anyone, that I write to describe it.

Two similar Leyden jars are joined up to similar fairly large loops of wire, one of the circuits having a spark-gap with knobs included, the other being completely metallic, but of an adjustable length.

The jar of this latter circuit has also a strip of tinfoil pasted over its lip so as to provide an overflow path complete with the exception of an air-chink, *c*. It is important that this overflow path be practically devoid of self-induction. A jar already perforated could be well utilized for the purpose.

Then if the two circuits face each other at a reasonable distance, and if the slider, *s*, is properly adjusted, every discharge of A causes B to overflow. A slight shift of the slider puts them out of tune.



Instead of thus adjusting by variable self-induction, my assistant, Mr. Robinson, has made a slight modification by using a condenser of variable capacity, consisting of two glass tubes coated with tinfoil, one sliding into the other, and joined by a flexible loop of wire; an easy overflow from one coat to the other being likewise provided. On making this loop face the discharging circuit of an ordinary Voss machine with customary small jars *in situ*, bright sparks at the overflow gap occur whenever the common machine sparks are taken, provided the sliding condenser be adjusted to the right capacity by trial.

There is little or no advantage in using long primary sparks; the vibrations are steadier and more definite with short ones. It is needless to point out that the 2 jars constitute respectively a Hertz oscillator and receiver, but fair precision of timing is more needed with these large capacities than with mere spheres or discs, because the radiation lasts longer and there are more impulses to accumulate. Hence actual resonance as distinguished from the effect of a violent solitary wave is better marked. Moreover, the sparks are bright enough to be easily seen by a large audience. OLIVER J. LODGE.

University College, Liverpool.

African Monkeys in the West Indies.

WITH reference to the note in NATURE of February 13 (p. 349), on the occurrence of an Old-World monkey in Barbados, I may point out that the same West African monkey (*Cercopithecus callitrichus*) has also been introduced and is now found wild in St. Kitts (cf. Sclater, P.Z.S., 1866, p. 79). It likewise

©1890 Nature Publishing Group

occurs in Nevis, whence the Zoological Society received living specimens (presented by Mr. Graham Briggs) in 1870.

The only West Indian island in which Quadrumana of the *American* type occurs is Trinidad, which was, doubtless, formerly part of the mainland of South America.

3 Hanover Square, W., February 17. P. L. Sclater.

Galls.

I had not intended to take any further part in this correspondence; but the interesting suggestion which has now been made upon the subject by Mr. T. D. A. Cockerell (Nature, Feb. 13, p. 344) induces me to withdraw the sentences that he quotes from my previous letters, to the effect that it seems impossible to imagine any way in which galls can be attributed to natural selection acting on the plants *directly*. In my own consideration of the matter this seemed "obvious," and therefore my motive in taking up the difficulty as presented by Mr. Mivart was that of "asking whether anybody else had a better explanation to offer" than the one which my letter suggested—viz. "that natural selection may operate on the plants *indirectly through the insects*," by always selecting those insects the character of whose secretions is such as will best cause the plants to grow the particular kind of morphological abnormality which the larvæ require. Mr. Cockerell, however, has now furnished what seems to me an extremely plausible hypothesis, showing that there is a way in which it is quite conceivable that the growth of galls may be an actual benefit to the plants, and therefore that natural selection may act directly on the plants themselves in evolving these sometimes highly specialized structures for the use of their parasites. Mr. Cockerell informs me in a private communication that he has been verifying this hypothesis by observations in detail; but whether or not he will be able to establish it, I think at any rate he has done good service in thus suggesting another possibility.

On the other hand, I cannot see that Mr. Ainslie Hollis has helped us at all (Nature, January 23, p. 272). For he merely enunciates the truism that trees which were not endowed with sufficient "developmental vigour" adequately to resist the attacks of gall-making insects "would doubtless have long ago succumbed in a struggle for existence." And this truism he appears to suppose furnishes an explanation of how "natural selection, operating in the ordinary manner," has produced galls for the exclusive benefit of the insects. But it is obvious that the more detrimental the growth of galls has proved to trees, the less reason there must have been for natural selection, "operating in the ordinary manner," to have developed these often highly specialized structures for the benefit of parasites.

London, February 13. George J. Romanes.

The Supposed Earthquakes at Chelmsford on January 7.

Nature for January 16 (p. 256) reprints from the *Essex County Chronicle* a short account of two supposed earthquake-shocks felt at and near Chelmsford on January 7, at 12.30 and 1.25 p.m. Being engaged in the study of British earthquakes, I made inquiries in the district referred to, and the result of these is to show that the shocks were almost certainly due to the firing of unusually heavy guns at Woolwich. It may be worth while to state the evidence for this conclusion somewhat fully, as it will be difficult to obtain it in after years.

(1) I applied to the authorities at Woolwich and Shoeburyness as to the nature of the firing on January 7. At the latter place, the only practice was from 9-inch and 10-inch guns, the maximum charge used was 70 pounds of powder, and therefore not capable of producing the shocks felt at Chelmsford. At Woolwich, however, the 110-ton gun, "*the heaviest in H.M. service*," was fired at the times mentioned.

(2) *Form of the Disturbed Area.*—The only accounts I have as yet received are from the following places: Great Warley (near Romford), Brentwood, Epping, Ingatestone, on the road between Ongar and Fyfield, Roxwell, Chelmsford, Chignall St. James, and Chipping Hill (Witham); which are respectively at about 6, 12½, 14, 16, 16, 21, 24, 24, and 32 miles distance from Woolwich. Referring to a map of Essex, it will be seen that these places all lie close to a line drawn from Woolwich in a north-easterly direction; with the exception of Epping, the direction of which is about north by east from Woolwich. According to the *Times* weather report of January 8, southerly and south-westerly breezes prevailed very generally throughout the kingdom on the previous day.

(3) *Nature of the Shock.*—In four cases, the shock was in the first instance attributed to the firing of heavy guns. If there was any vibration of the earth, it must have been very slight, and the following descriptions seem to leave little doubt that the rattling of windows noticed was due to an air-wave.

Great Warley—The shock "broke a pane of glass 4 feet × 2 feet on my job."

Brentwood—"The shocks commenced as a low rumble, increasing till the doors shook and rattled, as though the rumbling was followed by a bang or explosion."

Between Ongar and Fyfield (the observer driving)—"The ground felt as if it were sinking," and there was "a rumbling noise something like guns in the distance."

Roxwell—The sound "exactly resembled the report of the big guns at Shoebury, but was far louder than we usually hear them."

Chelmsford (the observer walking)—There was "a noise as of a very heavy weight being rolled across the floor of the room of the house to the south of him, which he was passing."

Chignall St. James—"The shock was extremely slight, but there was a most pronounced concussion in the air which made a sound on the windows as if a person had thumped the centre of the window frame with the soft part of his hand. There was no tremulous motion felt."

Witham—The observer "heard a strange rumbling sound which seemed to slightly deafen him, but he felt no vibration of the earth."

That the disturbances recorded had only one origin is, I think, evident, (1) from the decrease in intensity (roughly speaking) as the distance from Woolwich increases, and (2) from there being no considerable gap between the places of observation. Records from the immediate neighbourhood of Woolwich could hardly be expected, as there they would naturally be attributed to their proper source.

I am indebted to the editor of the *Essex County Chronicle* for inserting a letter asking for observations on the shocks, and to several gentlemen for the courtesy and kindness with which they replied to this letter and to other inquiries that I made in the surrounding district. Charles Davison.

38 Charlotte Road, Birmingham, February 13.

Shining Night-Clouds.

In July last, on a fine night, about 8 p.m. (two hours after sunset), I noticed a fleecy cloud lit up by a yellowish light, directly over the back of a range of hills due west from this place. As it did not move, it struck my attention, and I observed that what little wind there was carried the few floating clouds north-east to south-west. I continued to watch the cloud, which covered say 4° or 5°, until 11 p.m., and concluded that as in that direction lay the Puracé volcano, about 40 miles away, the light and cloud probably came from it. But I made inquiries by telegraph, and found that no eruption had taken place in the Puracé, which has been quiet now for many years. I regret, seeing now that the subject is interesting, that I did not observe more carefully. I may add that in the direction of the cloud no prairie or forest fire could have occurred to account for it. Robert B. White.

Agrado (lat. 2° 20′ N.), Department of Tolima,
U.S. of Colombia, S.A., December 22, 1889.

A Greenish Meteor.

To-night (Jan. 30), at 8.15 p.m., I saw a meteor which, notwithstanding a bright moon, shone out exceedingly brightly, exceeding any star. It appeared to travel south, for about 10°, vanishing about 15° above the horizon. Its colour differed from that of any meteor I have seen before, being pale green or greenish. T. D. A. Cockerell.

West Cliff, Custer Co., Colorado, January 30.

THE MOLECULAR STABILITY OF METALS, PARTICULARLY OF IRON AND STEEL.

(1) Allow me to add some words relative to the very timely lecture on the hardening and tempering of steel, recently published by Prof. Roberts-Austen

©1890 Nature Publishing Group